Letters to the Editor.

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Geology and the Nebular Theory.

THE literature of geology has grown so immense that no man can be familiar with all of it, particularly when it refers to another continent than one's own, yet it comes as a surprise to a Canadian to find eminent Old World geologists still referring to the nebular hypothesis as an established fact of geological history. A few weeks ago Prof. J. W. Gregory suggested that life began on mountains, since these were the first parts of the earth's crust to cool to a suitable temperature, and more recently Prof. Joly, in discussing the age of the earth, assumes the truth of the nebular hypothesis, though he admits that "there was indeed some scanty sedimentation in Archæan times."

Probably no country includes a larger area of Archæan rocks than Canada, and several parts of the area have been studied as carefully as possible because of their importance as mining regions, yet no evidence of a hot earth has been found. The Huronian rocks of cobalt include a glacial deposit which is known to have covered many thousands of square miles. The Sudbury or Timiskaming Series, next in age, consists almost entirely of sediments, such as boulder conglomerates which may be glacial, arkosi with unweathered feldspars, and graywacke with seasonal leanding. Near Sudbury the series has a thickness of more than 20,000 feet.

The oldest rocks of all are the Keewatin and the Grenville Series, the former consisting mainly of volcanics but including thousands of feet of sedimentary gneisses and of "iron formation"; the latter is made up wholly of sediments, reaching a thickness of more than 50,000 feet in places and containing immense deposits of limestone, as well as much carbon in the form of graphite.

It might be thought that the Keewatin lavas imply a hot condition of the earth, but as a fact most of them exhibit pillow structure, showing that they were poured out into water. Liquid water existed over many thousands of square miles, and probably the temperature was low enough for the life of algæ and perhaps of primitive animals, as suggested by the carbon and limestone. The rocks of this most ancient known geological period do not indicate a higher temperature than that of later times.

It is probable, however, that those geologists who think of the earth as hot in Archæan times have in mind the granites and gneisses which underlie the most ancient sediments, the Laurentian rocks of Canada and similar plutonic rocks of other countries, which undoubtedly are of eruptive origin, and have been described as part of the original crust of the molten earth. In reality the Laurentian batholiths are far younger than the sediments and volcanics of the Keewatin and Grenville which they have invaded, and a similar welling-up of plutonic batholiths has occurred at numerous times in the later history of the world, and is perhaps still taking place beneath great ranges of youthful mountains like the Andes and Himalayas.

The coast range of British Columbia, 1100 miles long and 100 broad, consists of just as characteristic batholithic rocks as the Laurentian, but is of Jurassic age, and the Andes, which are still younger, appear to be largely of the same character. Granite and gneiss may be of any age, and do not imply a cooling earth as some have supposed. We find greater areas of such plutonic rocks in the most ancient geological periods simply because they have been exposed to denudation for a longer time, and so have been more widely uncovered.

The conditions found in the Archæan of Canada are repeated in Brazil, India, and Scotland, and probably other countries of which the present writer has no personal knowledge. The oldest rocks in the world are sedimentary and indicate temperatures like those of later times. If the earth was ever a molten sphere, there is no evidence of this condition in the geological record, and geologists should not cling to an outworn theory which the astronomers themselves have largely given up. In the planetesimal theory a method of world building has been provided which permits of a cold surface from the beginning, and fits far better with the known geological facts than the nebular theory.

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University of Toronto, Toronto, Canada, May 16.

Species and Adaptations.

MR. BATESON'S address to the American Association at Toronto last December, which was published in NATURE of April 29, exhibits features of the same kind as those which were evident in his address to the British Association in Australia in 1914. In the Australian address he maintained that the effect of the discoveries and investigations in recent years in the phenomena of heredity and variation was greatly to increase the difficulty of understanding the origin of any characters which were new in the proper sense of the word. He went so far as to suggest that all characters which have appeared in the course of evolution may have been present in the protoplasm or nuclear structure of the original unicellular forms from which later forms, including man, have descended, all apparently new characters having been due to loss of inhibiting factors and segregation of various simpler combinations from the original complex. Now Mr. Bateson again declares himself an agnostic with regard to the evolution of species, and in spite of all modern discoveries, or because of them, states that we are farther than ever from any satisfactory explanation of the evolution of a new species, or of two or more species, from a single ancestral species.

Mr. Bateson admits that plenty of Mendelian combinations would in nature be given specific rank, and then proceeds to state that the topic of evolution is now dropped in genetical circles. He then illustrates the rule of silence on this favourite subject of a former generation by devoting the rest of his discourse to it, only to lead up to the conclusion that specific difference probably "attaches" to a base of which we know absolutely nothing at all. Our faith in evolution, Mr. Bateson declares, is unshaken; our doubts are merely as to the origin of *species*.

Now I have no intention of stating in opposition to Mr. Bateson that our present knowledge fully explains the origin of new species; I wish merely to offer some criticisms of the difficulties which he describes. In the first place, I dislike the expression "faith in evolution." I do not share the distrust in facts and reasoning which is now in vogue as a reaction against the excessive confidence of the nineteenth century. Evolution is a question of science, of verifiable facts and sound reasoning, and has nothing to do with faith. Mr. Bateson himself in another paragraph states that we have absolute certainty that new forms of life, new orders and new species, have arisen on the earth. The explanation is the difficulty, but we have ample evidence that organisms, whatever their characters, are only produced by reproduction from parents.

One is tempted to conclude that Mr. Bateson attaches some mystical meaning to the word "species." He says we have no reason to suppose that any accumulation of characters of the same order as those met with in genetical experiments would culminate in the production of distinct species. According to him there is some underlying base which is specifically distinct and bears the characters. I fail to see that this idea has any scientific meaning or validity. What is this base? In science we must regard things and phenomena objectively. We distinguish species by characters, just as we distinguish all objects by their qualities. For example, we have the familiar example of single comb and rose comb in fowls. We cannot have singleness and rose-ness without the comb that exhibits these characters. If they occurred in nature, excluding crossing or hybridisation, they would be specific characters, at least in company with other differences they might be. Is the comb then the base? The comb must have some character and shape, and thus we cannot have the comb without a character. We can have the entire absence of comb, as in the allied genus Phasianus; and so with all other characters. This idea of a specific base distinct from specific characters seems merely false metaphysics. How can we conceive of an organism without characters, or characters without an organism? Perhaps Mr. Bateson means that unit characters such as those which can be transferred in Mendelian crosses might all be taken away, and still an organism would be left with non-Mendelian characters. What are these characters? He does not tell us. We have cases of the absence of pigment in, *e.g.*, a bird, then the feathers are left. We may have an organism without feathers, and then the skin is left. We can scarcely have an organism, at any rate a vertebrate, without a skin. On the other hand, we may have factors, whatever their nature, which in the absence of one or more other factors produce no visible character, as in the cases of white varieties of animals and plants which, when bred together, produce coloured offspring. It has been shown that there are several kinds of white varieties or races, the distinguishing characters of which are invisible. Perhaps Mr. Bateson means that species were originally distinct in this way, separated by characters which were non-apparent.

Mr. Bateson insists on the rarity of the occurrence of new dominants under observation in experimental breeding, although new recessives, that is, the loss of particular characters from a combination, are common enough. Even in Drosophila few new dominants have been seen, and none of these could be expected to survive under natural conditions. He further states that in tracing the origin of our domesticated animals and plants we can scarcely ever point to a single wild species as the probable progenitor. Now it seems to me that there is very good evidence that all our breeds of domesticated fowls have descended from *Gallus bankiva*, and in the numerous existing breeds there are many dominant characters which are not present in the wild ancestral form, *e.g.* the dominant white of the White Leghorn, and the rose comb. Mr. Bateson says he cannot imagine such a new dominant character being produced. But surely it is evident that they have been produced in the succession of generations of domestic fowls. Mr. Bateson's difficulty seems to be merely that we do not know how they came into existence. We can, however, scientifically form the conclusion that they originate by some change or development in the chromosomes, not directly dependent on any corresponding external stimulus.

Another reason which Mr. Bateson gives for his scepticism is that the chief attribute of species is that the product of their crosses is frequently sterile. This seems on the face of it illogical. If the sterility is only frequent it follows that there are many cases in which such sterility is absent. In that case, as there are many species which produce fertile offspring, the sterility of species hybrids cannot be the "chief attribute" of species. It is neither a universal nor necessary characteristic, and all we can say is that we do not know how it arises in certain cases. John C. Phillips in America crossed three wild species of duck (*Anas boscas*, *A. tristis*, and *Dafila acuta*) and found the progeny fertile. Bonhote has published the results of numerous similar experiments in this country. The various species of Bovidae also are stated to be fertile *inter se*.

I find it very difficult to understand Mr. Bateson's reasoning on this subject. He states in one place that the fact that hybrids between species are by no means always sterile is a commonplace of everyday experience, and then a little farther on, insists that until the production is witnessed of an indubitably sterile hybrid from completely fertile parents which have arisen from a single common origin, we have no acceptable account of the origin of "species." The two statements contradict each other. Interspecific sterility may be very mysterious, but it has nothing to do with the origin of these species which do not exhibit this sterility. Moreover, there is evidence of the occurrence under observation and experiment of new varieties which are more or less infertile with one another. *Œnothera gigas*, a mutant from *O. Lamarckiana*, shows a great degree of sterility when crossed with other mutants from the same species, and two mutants of Drosophila in Morgan's experiments are almost completely sterile with one another.

It is not very surprising that genetical researches of the Mendelian kind have not thrown much light on the occurrence of variations and mutations, for except in the cases of Œnothera and Drosophila they have usually consisted in analysing by crossing experiments the hereditary factors already present, instead of breeding many generations from a single form and studying the variations that occur. To my own mind, there is no proof that the numerous breeds and varieties of domestic fowls, all descended almost certainly from the single species *Gallus bankiva*, differ in their essential nature from groups of closely allied species and varieties in a natural state.

The feeling, however, that chiefly prompts me to comment upon Mr. Bateson's Toronto address is one of protest against the implied disparagement of those who have not ceased to discuss evolution. There is more in evolution than the origin of species. Mr. Bateson himself has contributed largely to the proof that the distinctions between species have little or nothing to do with adaptation, but at the same time he has failed to realise the true nature and importance of adaptation in itself. In his address he makes no reference at all to adaptation, or to the relation which it bears to recapitulation in ontogeny, one of these "academic problems of morphology" which were discussed with such avidity when both he and I were young, and which he relegates with such confident assurance to the limbo of obsolete things. Yet he writes of the older time: "Regardless of the obvious consideration that 'modification by descent' must be a chemical process, and that of the principles governing that chemistry, science had neither hint nor surmise nor even an empirical observation of its working."

One would almost suppose that Mr. Bateson was a bio-chemist. But how much chemistry is there in the analysis of Mendelian factors, or the identification of spots in chromosomes which represent particular genes ? The suggestions of the nature of the " chemical process " have come from the physiologists and from those who, without ignoring the methods and discoveries of genetics, have not ceased to discuss evolution and adaptation. It is true that some geneticists have discussed the question whether factors for colour might be chemical compounds reacting on each other, but they have not explained how chemical compounds such as enzymes and chromogens could be contained in separate chromosomes and segregate from each other in the reduction divisions of gametes. I do not remember any case in which " modification by descent," that is the loss or gain of a unit character, has been shown by geneticists to be due to any chemical process. The latest results of the American investigators concerning the localisation of genes in the chromosomes, concerning which Mr. Bateson states that all his scepticism has been removed, are purely morphological.

All the progress that has been made in our knowledge of unit characters and of specific characters has tended to exhibit more and more clearly the difference between such characters and adaptational features. It is seldom that an adaptation is confined to a single species, and it is impossible to perceive any connexion between mutations or unit characters and the relation of adaptations to function and external conditions. One great event in the evolution of both animals and plants was the adaptation of the descendants of aquatic forms to terrestrial and atmospheric conditions. In the case of animals, we have, in the metamorphosis of Amphibia and the embryonic development of higher vertebrates the recapitulation of this transition from aquatic organs of respiration to atmospheric organs, not by conversion but by substitution. It is certain from this evidence that the change was perfectly gradual and continuous, and parallel to the gradual change of conditions and mode of life. Recapitulation in this case, however ancient a subject it may be, is an obvious fact, and nothing that the geneticists have discovered throws any light on it, or diminishes its importance. It is no use dismissing it as early Victorian. The question is, have the recent, much vaunted discoveries explained it, or have they anything to do with it ? Variations in wings, eye colour, etc., of flies bred in milk-bottles are important in their own sphere, but they throw no light on the annual growth, denudation, death, and recrescence of the antlers of a stag, or on the remarkable relation between these processes and the hormones from the gonads. The origin of species is a very important problem, but it is not the whole, or the most important part, of evolution. The origin of adaptations is not the same problem as the origin of species, and the methods of modern genetics have very little bearing upon it. Mr. Bateson's address suggests that he has not yet realised the difference between the two problems, or paid serious attention to modern physiological knowledge bearing on functional adaptation. The phenomena of recapitulation, so closely associated with adaptation, imply wherever they occur a continuity in the evolutionary change of which the adaptation was the result, and these phenomena are quite incompatible with the discontinuity which is characteristic of non-adaptive variations, and which is the cardinal principle of Mendelians and mutationists.

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Evolutionary Faith and Modern Doubts.

No one can have read without interest Dr. Bateson's admirable address on evolution published in NATURE of April 29. While Dr. Bateson's reputation is justly high and his views necessarily command respect, it must be admitted that some of his arguments are very difficult to follow. When, for example, he says that " the conclusion that species are a product of a summation of variations, ignored the chief attribute of species, that the product of their crosses is frequently sterile in greater or less degree," I am frankly puzzled. The proposition is certainly not self-evident. If a sword and its scabbard are bent in different directions, it will happen sooner or later that the sword cannot be inserted, and the result will be the same whether the bending be effected by a single blow, or whether it be, in Dr. Bateson's words, " a product of a summation of variations." Is this illustration inapt ? The sword and its scabbard are the homologous chromosomes. These presumably have to co-operate to produce the somatic cell of the hybrid, and their co-operation might be expected to require a certain resemblance, but for the production of sexual cells they must do more, they must conjugate ; and for conjugation it is surely reasonable to suppose that a much more intimate resemblance would be needed.

We might, therefore, expect, on purely theoretical grounds, that as species and genera gradually diverged, it would be increasingly difficult to breed a hybrid between them ; but that, even while a hybrid could still be produced, a fertile hybrid would be difficult or impossible, since the cells of the germ-track would fail to surmount the meiotic reduction stage, when the homologous chromosomes conjugate. This is exactly what happens : the cells go to pieces in the meiotic phase.

It would even seem that the argument is exactly contrary to Dr. Bateson's statement of it : it seems easier to imagine sterility arising from a gradual modification, spread over a length of time, and involving many chromosomes, than from the half-monstrous variations chiefly studied by Dr. Bateson and his school, variations which appear to affect only a few chromomeres, and those by loss alone.

Now I certainly cannot pretend to much or special knowledge, either in genetics or cytology. But I would ask Dr. Bateson in all humility whether there is any difficulty involved in this simple solution of his problem. Very likely there is, but he does not indicate it.

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Transcription of Russian Names.

IN NATURE of May 20, p. 648, is published a letter from Maj.-Gen. Lord Gleichen, who raises objections to Prof. Brauner's suggestion (NATURE, April 29) that we should adopt the Czech transcriptions for the names of Russian men of science.

The argument that there are typographical difficulties is surely a very small one, since NATURE and other journals (*e.g.* that of the Chemical Society) already employ letters with diacritical marks in writing the names of Czech and other authors. Whilst diacritical marks are undesirable for place names on maps, the same need not apply to the names of persons.

The main points raised by Prof. Brauner in support of his suggestion, remain unchallenged, and in addition to these it may be mentioned that the Czech language is phonetic and Russian names can thus be accurately pronounced according to it.